sary quality and are then put on the market, it appears that there will not be any great risk of foreign competition in the matter of price. Indeed, according to several correspondents, there is little to be feared at the present time. But opinions are not in agreement.

There should be no objection to "manufacturers' associations," provided that their object is to obtain the advantage of more economical methods of manufacture, as by uniformity of standards and large-scale production, rather than the maintenance of high prices.

The cost of all research work, whether paid for by Government grants or otherwise, is greatly increased by inferior apparatus. At the present prices of materials, a single biochemical preparation may cost 4*l*. or 5*l*. or more. This may be lost by breakage at the final stage. The question naturally arises whether economy would not be effected by allowing free import, even at the cost of subventions to British makers.

With reference to Mr. Watson Baker's statement (NATURE, June 24, p. 518) that there are 12,000 German binoculars in London, I confess that I had chiefly in mind the use of apparatus in teaching and research. The sale for general use certainly raises a difficulty. As to losses incurred by work done for Government Departments during the war, so far as my information goes payment for these did not err on the side of economy. Liability for excess-profit duty surely implies that the profit has been made.

The statement by Mr. C. Baker (NATURE, May 20, p. 356) that capitalists will not put money into the business raises another question. It may well be that British makers do not find it profitable to undertake the supply of fine chemicals and special apparatus used only in small amount, even apart from foreign competition. If so, why not give up the trade to those who make a profit on the sale?

The desire of the British industry for prohibition of import appears to rest chiefly on the fear of competition by Germany. I am not one of those who imagine that because an instrument is of German origin it is necessarily superior to all others. Indeed, I have heard of instruments verified at Charlottenburg being found inaccurate. It would certainly be less obstructive than total prohibition if the restriction applied to German goods only. But there are other considerations to be remembered here, such as the importance of giving an opportunity to that country to restore its credit. However this may be, the large profits of their chemical industries referred to above raise some doubt as to the real cause of the present unsatisfactory conditions in Germany.

The point raised by Mr. Dunkerly that American microscopes and lenses are being sold here, although the rate of exchange is against us, suggests that the source of the trouble is not the low value of the German mark. This view is confirmed by other correspondents. If it is correct, there would be no real gain in a mere prohibition of import. Improvements in modes of manufacture are needed, and we come back again to the necessity for more scientific research.

I note that the British Optical Instrument Manufacturers' Association (NATURE, May 20, p. 355) considers that a tariff might have the result of removing the inducement to improve quality, but I foresee so many difficulties in the way of convincing a Government official that a particular piece of apparatus could not be obtained in England that I am unable to accept the suggestion of import by permit as a satisfactory alternative. If, however, it were possible for every scientific worker to obtain without difficulty a general permit for the import of any apparatus at any time the situation would be different.

There seems to be much doubt as to whether it is really possible to obtain foreign apparatus at a price much lower than the British. Should this be the case, the payment of a subsidy might be considered where there is actual underselling. The test would then become one of quality.

The importance of the subject may, I think, serve as an excuse for this lengthy letter. Scientific workers have every desire to assist the development of the industry, but they feel that they are not justified in wasting time and money where it could be avoided. And if this correspondence has brought out the fact that satisfaction has not yet been given in the matter of quality, especially in the case of certain goods, it will have been of some value. It is possible that users have not sufficiently made known their difficulties to the makers, and have been sometimes content with the purchase of foreign material when further inquiry and discussion might have enabled British goods to be forthcoming. W. M. BAYLISS.

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The Separation of the Isotopes of Chlorine.

MR. D. L. CHAPMAN's argument appears essentially to be similar to that already developed from a quite different point of view by Lindemann (*Phil. Mag.*, 1919, vol. xxxvii., p. 523; vol. xxxviii., p. 173), that because isotopes are (theoretically) separable by physical means, they must also be chemically separable according to thermo-dynamical reasoning. The fact that the particular mode of separation by semi-permeable membranes (assumption (3), NATURE, July 15, p. 611) is highly fanciful need not obscure the nature of the argument. Lindemann's conclusion that, though isotopes cannot be identical chemically, the difference may be reduced to an unmeasurable one of the second order of magnitude by suitable assumptions as to the "Nullpunktenergie," seems to indicate the more hopeful line of advance. The chemical non-separability of isotopes, of which there is an accumulated mass of experimental evidence, seems to call for consequent adjustments in thermo-dynamic theory rather than the reverse.

The following considerations may throw light on the matter. I have stated (NATURE, June 24, p. 516) that, on the assumption of the chemical identity of the isotopes, the distribution given by probability considerations of the two kinds of atoms among the three kinds of molecules is

$$\mathrm{Cl}_2 : \mathrm{Cl'}_2 : \mathrm{Cl.Cl'} :: n^2 : (1-n)^2 : 2n(1-n) \quad . \quad \text{(i)}$$

where n and $(1-n)$ are the fractional proportions of the Cl and Cl′ atoms respectively. This leads to the equilibrium condition

$$[\mathrm{Cl}_2][\mathrm{Cl'}_2]=\tfrac{1}{4}[\mathrm{Cl.Cl'}]^2 \quad . \quad . \quad . \quad . \quad \text{(ii)}$$

Now if one applies in the conventional manner this result to the reversible reaction

$$\mathrm{Cl}_2+\mathrm{Cl'}_2 \rightleftharpoons 2\mathrm{Cl.Cl'},$$

denoting by k_1 and k_2 the coefficients of velocity of the direct and the inverse reactions, one gets

$$k_1 n^2(1-n)^2 = k_2\{2n(1-n)\}^2$$

or

$$k_1 = 4k_2.$$

This, to say the least, is unexpected, because if coefficients of velocity of reaction have any physical significance at all, one would expect them to be the same for substances assumed to be chemically identical. The result is clearly due to a loose method of

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choosing the concentrations, for if we re-write the reversible reaction

$$Cl_2 + .Cl'_2 \rightleftharpoons Cl.Cl' + Cl.Cl'$$

it transpires that we have chosen for the concentration of the resultants, because they are the same, the sum of their individual concentrations, although for the reactants, which also are chemically the same, the individual concentrations have been taken. It is clear that it is the individual concentrations in both cases that have to be taken, and therefore that one-half of the Cl.Cl′ concentration is involved. Then $k_1 = k_2$. So with any reaction of this type involving two molecules, apart from the question of isotopes altogether, the 4 that always appears in the conventional text-book examples is merely a consequence of a loose and physically unjustifiable mode of representing the concentrations. Writers of future text-books might ponder a little over this. If the same change in the choice of the concentrations is made in the thermodynamical argument, the difference of entropies, $R \log_e 4$, reduces apparently to $R \log_e 1 = 0$.

I have now made some progress in the application of probability considerations to the kinetics of the reaction. The distribution already given (i) has reference merely to the manner in which the two kinds of atoms will arrange themselves among the three kinds of molecules, assuming promiscuous combination between the two kinds, the two kinds being identical. But the particular distribution obtained does, I find, depend upon the kind of recombination assumed. As regards the dissociation of the molecules into atoms prior to their recombination, the matter appears straightforward, at least so far as I have got. Thus whether one supposes that in a certain time a certain fraction of collisions, the same on the average for each kind of molecular collision, is fruitful in dissociating the two molecules into four atoms, or one regards the dissociation as monomolecular, as presumably it would be if light were the dissociating agent, one arrives at the same result, that if x, y, and z denote fractional proportions of Cl_2, Cl'_2 and Cl.Cl′ molecules respectively $(x+y+z=1)$, the relative rate of disappearance of each by dissociation is similarly denoted. By equating this rate of disappearance to the rate of formation for the three kinds of molecules, one gets the equilibrium distribution. The distribution given by (i) is got in this way, whether (1) all the atoms of the two kinds recombine promiscuously or (2) the four atoms formed in a single fruitful collision recombine again only among themselves. If a similar limitation be applied to a monomolecular dissociation, obviously the reaction cannot affect the distribution at all, which remains unchanged whatever the initial distribution. But I also found by inadvertently applying the law of promiscuous recombination separately to each of the nine cases that have to be taken into account on the collision view—since there are three types of molecules which may collide with any one of the three types—instead of to the sums of each of the two kinds of atoms produced, that a very extraordinary equilibrium distribution resulted, given by

$$Cl_2 : Cl'_2 : Cl.Cl' : : \tfrac{1}{3}n(1+2n) : \tfrac{1}{3}(3-2n)(1-n) : \tfrac{4}{3}n(1-n),$$

which leads to the curious concentration equation

$$[Cl_2][Cl'_2] = \tfrac{1}{4}\{[Cl.Cl']^2 + [Cl.Cl']\}.$$

This in the case $n=0{\cdot}5$ happens to reduce to Mr. Chapman's relation (i) (NATURE, June 17, p. 487).

The case, of course, has no physical meaning, but it may serve to show that the equilibrium distribution is sensitive to the particular assumptions made as to the type of reaction which occurs. I do not imagine I have exhausted the physical possibilities, but, so far as I can see, my distribution relation (i) covers the physically conceivable cases, and therefore the half, not the whole, concentration of the substance undergoing a bimolecular reaction with itself ought to enter into the equilibrium equation.

FREDERICK SODDY.

Science in Medical Education.

THE discussion at the British Medical Association on July 1 on the place of preliminary science in the medical curriculum seemed to indicate practical unanimity on some points, such as the need for a higher minimum standard of general education, the raising of the minimum age for the registration of medical students to seventeen years, and the necessity for the maintenance of a high standard of instruction in physics, chemistry, and biology. There was no indication of the desire on the part of any one of the speakers to reduce the present standard of requirements in any one of these three fundamental sciences, and several suggestions were put forward for extending the courses of each of them into the later years of medical study.

Particularly welcome to many of the science teachers who were present were the remarks of Dr. Brackenbury, who insisted that a high standard of scientific education was just as necessary for the general practitioner as it is in the case of any specialist, and that consequently, in so far as the preliminary science courses are concerned, there should be no division of the courses into a higher and lower standard for different classes of medical students.

On the question of the relegation of the science courses to the school period of the student's education there were some minor points of difference of opinion, and there is need for further consideration of this matter and for the development of a common plan of action. If by raising the age of registration to seventeen years the school period is increased by an average of one year, there will be time for some school instruction in the fundamental sciences after the student has passed a matriculation examination without science, and there can be no doubt that if this time is profitably used, so that the student gains some knowledge of the elementary facts and principles of the sciences, the courses in the first year of study at the universities can be so modified in form as to bring home to the student much more forcibly than the courses do at present the relation of pure to applied science in medicine. The very prevalent idea that a great deal of time is wasted in the first year at the university in learning science that has no application to medicine arises entirely from the fact that the majority of the students come to the university so ignorant of elementary science and so untrained in scientific thought that the time of the university teachers is wasted in teaching the most elementary principles that could and should be taught at school. It seemed, however, to be the general opinion of those who were present at the meeting that the teaching of chemistry, physics, and biology should not cease at the end of the school period, but be extended into the first year of university study in a form which would be more general as regards principles, and more specialised as regards its application to the medical sciences. The suggestion made by Prof. Lorrain Smith and other speakers, that the teaching of science should be extended into the later years of the medical curriculum so that the links that bind the pure sciences to the medical sciences should be continuously presented to the medical student, does

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